A COMPARISON WITH THE BAROMETRIC STANDARD OF COSTA RICA.

In a letter of August 13 Mr. H. Pittier states that he had lately sent one of his assistants to Limon, Costa Rica, with a new barometer that had been carefully compared with the standard of the Physico-Geographical Institute. The mean result of ten observations taken hourly during two days (July 29 and 30) showed that the barometer at Limon read too high by 6.54 mm., and that this error had existed for several months. The instrument is an old French model with a broad cistern and Fortin's adjustment for transportation. The error was largely due to the fact that before being transferred from one room to another, several months ago, the cistern had been properly screwed up, but had not afterwards been completely screwed down. Even after the latter operation had been properly performed on July 29 by the assistant, there was still need of a correction of 0.6 mm. Consequently, the faulty barometer was brought back to Costa Rica and the other one left in its place; this latter was constructed by James Green, of New York, and left at San Jose by William Gabb, but subsequently repaired by Negretti and Zambra; its readings may be relied on.

The international comparison of the barometric standards of various European countries and the United States, executed by Prof. Frank Waldo in 1885, needs now to be renewed and extended to include all the American States. General meteorological studies require that the pressure should be known to within 0.01 inch at all important stations in the Northern and Southern Hemispheres.—*C. A.*

PHYSICS AND METEOROLOGY.

A recent letter from a correspondent says:

In the course of two or three years teaching of physical geography, etc., many questions have arisen, for some of which I have found no satisfactory or authoritative answers. I do not know whether it is your custom to answer such questions either privately or through the columns of the REVIEW, but I take the liberty of submitting the following queries:

1. What effect, if any, does the increasing density of the earth's atmosphere, near the earth's surface, have upon the amount of insolation absorbed by the atmosphere? I do not find this given in the meteorologies as a cause for unequal heating of different strata of air. I have wondered if it has any effect.

2. Ganot's Physics, page 412, says that ordinary undried, but not especially moist, air was found in a certain experiment to absorb 72 times more heat than dry air. Davis's Meteorology says that water vapor is found by experiment to be as poor an absorber as dry air. Are not these statements contradictions and which is correct? To what experiments does Davis refer?

3. Is there any clear explanation why increasing the temperature of a space increases the capacity of that space for vapor? If I understand correctly, a space becomes saturated when the vapor pressure becomes such that for every molecule of water forced into it from the evaporating surface one is forced out. When this equilibrium is attained would it not seem that raising the temperature of the vapor in the space would, by causing an increase in the vibration of the vapor molecules, cause a greater expansive force and thus prevent rather than allow more vapor to pass in? Of course experiment proves that more will pass in and can exist in the vapor state when the temperature is raised, but is there any explanation for it?

4. Can the article published in the REVIEW some time ago on the "Gulf Stream Myth" be regarded as authoritative? Of course the influence of the stream has been overestimated, but does not this article underestimate its effect? Davis seems to think the ocean currents are important enough to attribute to them the cause of the deflection of the isotherms. Is he correct in this or has he underestimated the influence of the winds?

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The following replies are published as being of general interest:

1. In reply to the first question, it may be stated that the amount of heat absorbed by a layer of gas of given thickness is proportional to the thickness of the layer and its transparency; the latter depends on density and dust or haze, therefore a layer of air one foot thick absorbs more when near the earth's surface than when high up in the atmosphere. But for dustless air this amount is so small that it will not account for the unequal warmth of the different strata of air, since clean, dry air is exceedingly diathermanous.

The following quotation from Prof. F. W. Very's article on the "Solar Constant"[1] will, perhaps, elucidate this subject:

It is commonly supposed that the larger portion of the heat produced by the absorption of the solar rays remains in the lower layers of the atmosphere, because these are richest in the vapor of water and in dust. See, for example, M. Crova's Mesure de l'intensité calorifique des radiations solaires et de leur absorption par l'atmosphère terrestre, p. 1, Paris, 1876. M. Radau, Actinométrie, p. 12, says: "In proportion as the rays penetrate into the atmosphere they encounter layers more and more dense, and the loss which they experience through unit path is proportional: (1) to the actual intensity of the beam; (2) to the density of the layer which they traverse; (3) to a constant coefficient of absorption * * * which varies with the nature of the rays." On page 14 Radau says: "The absorption is due in great part to the vapor of water distributed in the lower layers of the atmosphere." Although it is recognized (page 18) from the observations of Desains that the ratio of long-wave solar radiations on a high mountain to those at sea level must diminish when the air is very moist, nevertheless no objection is made to the use of formulæ in which the aqueous component of the absorption is assumed to be proportional to the density of the aqueous vapor.

The actual case is much more complicated. Selective reflection increases in the lower atmospheric layers, but does not warm them. Low layers of a moist atmosphere become hot because they absorb the rays of extremely long wave-length emitted by the heated soil. The sun heats these layers indirectly by first heating the ground, but contributes little heat directly, since the rays absorbable by aqueous vapor have been nearly all sifted out of the sunbeam before this reaches the lower atmospheric layers. On the other hand, the higher atmosphere, which contains a smaller quantity of aqueous vapor, is the first to attack the incoming rays. It is in the upper layers that the aqueous absorption of the solar infra-red rays takes place chiefly, and these are therefore the layers which are most warmed by the direct rays of the sun. I have noted elsewhere (Atmospheric Radiation, p. 123) that after rising above the comparatively thin layer of convectionally heated air, that portion of the diurnal range of temperature due to the immediate absorption of the solar rays may be expected to increase up to nearly the limit of the aqueous atmosphere, and it is surmised that this variation may possibly approach a 10-fold ratio of that which occurs at altitudes of one or two kilometers.

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2. In reply to the second question experts have differed widely in their statements as to the diathermancy of aqueous vapor. Preston, in his Theory of Heat,[2] says:

Experiments of Lecher and Pernter.—More recently a series of experiments on the absorption of radiant heat by gases and vapors has been published by Ernest Lecher and Joseph Pernter,[3] but these new investigations, instead of settling the question in dispute between Tyndall and Magnus as to the comparative absorptions of dry and moist air, place the whole matter in a state of greater uncertainty. For whereas Tyndall found an exceptionally low absorption for dry, and a high absorption for moist air, while Magnus found the same absorption for both, and that tolerably high, the results of the experiments of Lecher and Pernter show particularly no absorption for either, or, in other words, both dry and moist air act as a vacuum toward radiant heat.

It may be safely accepted that aqueous vapor energetically absorbs only special wave-lengths in the spectrum, but so does dry air absorb other waves. If these special waves happen to be contained in the beams of radiation on which laboratory experiments are being made the results of measurements of absorption will be quite different from measurements made on other beams that do not contain the special wave-lengths. It is quite plausible that the differences between different experimentalists and between the statements in the different works on physics are due to differences in the character of the radiations that have been experimented with. Professor Langley's work for the last twenty years has been devoted to measurements which it is hoped will clear up these discrepancies.

The quotation given above from Professor Very indicates a very considerable absorption of solar radiation by the aqueous vapor in the upper atmosphere. Professor Very's conclusions

[1] Monthly Weather Review for August, 1901, Vol. XXIX, p. 364.

[2] The Theory of Heat, Thomas Preston. London and New York, 1894, pp. 485-486.

[3] Lecher and Pernter. Sitzb. der k. Akad. der Wissenschaft in Wein, Juli, 1880; Phil, Mag., January, 1881.